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How Do Firms Feel About Participation by Their Peers in the Regulatory Design Process? An Online Survey Experiment Testing the Substantive Change and Spillover Mechanisms

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Abstract. This paper examines two concerns with calls for increased participation by firms in the government design of regulations in emerging markets. First, given the profit maximization goals of firms, can the benefits of business participation be realized without weakening the social protections that regulations are meant to offer? Second, can resource-constrained states realistically use consultation programs to influence the behavior of enough relevant firms to achieve a reasonable breadth of social protection? We explore these understudied questions through an online survey experiment involving 121 firm managers in Ho Chi Minh City, Vietnam. We find that firms prompted to think about worker safety favored regulatory changes that arose from suggestions by other firms over those resulting from suggestions by chemical safety experts even when unaware of the involvement of either source in the design process. By contrast, firms prompted to focus on business success did not express significant support for revisions suggested by either firms or experts. Learning about the participation of fellow firms positively influenced views about the legitimacy of the state's regulatory authority but not of the focal regulation itself.

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Keywords: regulation • regulatory compliance • participation • consultation • SMEs • Vietnam

1. Introduction

Global economic integration and multilateral trade agreements have expedited the diffusion of regulatory standards from advanced economies into emerging and frontier markets (Mattli and Büthe 2003, Busch et al. 2005, Hassel 2008). However, regulatory upgrading has proven difficult to implement in bureaucracies with limited technical knowledge and enforcement capacity (Kroszner 1998, Jordana and Levi-Faur 2004, Radaelli and De Francesco 2013). The awkward fit between transplanted regulations and on-the-ground, operational realities in individual emerging markets has further exacerbated these implementation challenges. For these reasons, analysts have suggested that participation by local firms in the regulatory drafting process could assist in tailoring new measures to local conditions and thereby improve their quality (Gillespie 2008, Farrell and Newman 2014).

The World Bank's 2017 *World Development Report* further suggests that, in emerging markets, consulting firms during the design of government regulations can improve business perceptions of state authority and reduce dangerous regulatory noncompliance (World Bank 2017). Even in China's communist single-party context, research indicates that support for government and its policies grows with participation during the rule-making process (Fishkin et al. 2010, He and Warren 2011, Truex 2014). This potential for political participation by firms to improve their behavior offers much needed hope to citizenries seeking protection from the negative externalities of rapidly expanding business activities.

However, important questions remain about the effect of broad-based business participation on ultimate levels of social protection and about the expense of such policies in emerging markets. Specifically, given the natural, profit-maximizing motives of firms, is it possible that consultation would serve as a Trojan horse, disguising a counterproductive regulatory race to the bottom? Additionally, is it truly feasible for resource-constrained governments to consult all firms covered by each new regulation?

The first of these two questions relates to the fact that a firm may suggest specific changes to a draft regulation that, if government accepts them, will reduce the firm's costs of subsequent compliance, thereby increasing its profitability. In this paper, we refer to this as the *substantive change mechanism*. Importantly, there are two sharply contrasting versions of this mechanism through which business participation can increase regulatory compliance. First, poorly informed officials may agree to suggestions by profit-maximizing firms that weaken constraints on activities harmful to society. We refer to this as the *weaker law mechanism*. Second, business participation can improve officials' understanding of technical details, equipping them to think in more nuanced ways about how to overcome important operational challenges to their social protection goals. We call this the *better law mechanism*.

For theoretical clarity, we present these two versions of the substantive change mechanism as ideal types. In practice, however, neither version is likely to be mutually exclusive or even fully separable in the minds of the firms providing comments or judging regulatory changes. It is entirely possible for firms to believe that regulatory changes simultaneously make factories safer and enhance business performance. Indeed, we see evidence in the actual regulatory changes we study in this paper that such a view would be entirely appropriate for firms to hold as well as evidence in our online experiment that this is the way that many firms do interpret actual changes to regulation.

Our second question relates to a substantial practical challenge in exploiting the potential of the participation–compliance relationship. Simply put, it is not realistic for resource- and capacity-constrained emerging market governments, often plagued by corruption, to involve all relevant firms in the design of every important regulation. As a result, even if all of the positive arguments about business participation are correct, governments must still consider significant cost–benefit questions. In most realistic scenarios, participants comprise only a small share of the firms in the risk set for noncompliance. If only this subset of firms alters their odds of compliance, the effects of participation on the ultimate outcomes that society cares about—labor safety, product quality, safety and sanitation standards, and environmental conditions—may be limited, at best. However, in most emerging markets, the alternative of a general policy of consulting all firms covered by a new regulation is likely to be prohibitively expensive.

Fortunately, research has found that firms are heavily influenced by the actions and beliefs of other firms that are similar to them (Meyer and Rowan 1977, DiMaggio and Powell 1983). This is particularly true under the conditions of heightened uncertainty (DiMaggio and Powell 1983) that typify emerging markets. As such, there is reason to believe that firms may be more likely to comply with regulations that they learn were shaped by input from similar businesses. This could be because it makes them more likely to see compliance as a source of institutional legitimacy or because firms believe that firms like themselves (i.e., with similar sizes, sectors, and owner backgrounds) will have represented a perspective like their own when interacting with regulatory authorities. Either way, this firm-to-firm influence points to the potential for government to leverage what we term the *participation spillover mechanism*. By this, we mean that the state can use consultation of a randomly selected minority of the relevant population of firms to influence the thinking and compliance behavior of the remaining majority of firms. Better understanding of this mechanism could improve the design and cost effectiveness of participation within the regulatory design process and, ultimately, make it more feasible as policy.

In this paper, we explore these two research questions through a multilayered online experiment involving the leaders of 121 firms in Ho Chi Minh City (HCMC), Vietnam. We asked respondents for their views regarding authentic revisions to a draft regulation, some of which came about through input from other firms, and their beliefs about the legitimacy of state regulators. Beyond this, the experiment was composed of three distinct and crosscutting experiments. First, in a within-firm design, we disguised whether firms were judging regulatory changes that had come about because of feedback through notice and comment by a broad cross-section of fellow firms as opposed to the status quo approach to regulatory drafting in Vietnam, which formally only involves state officials receiving input from safety experts. This design allowed us to identify whether business participation altered the regulation's language in idiosyncratic and hard-to-detect ways that, on average, made the revisions more acceptable to nonparticipating firms. Second, we randomly assigned firms to prompts that asked them to consider either business performance or worker safety when answering questions. We used firms' answers about whether regulatory change improved performance to adjudicate the relative weight of the weaker law mechanism, and those assigned to consider worker safety allowed us to judge the weight of the competing better law mechanism. Importantly, we designed our study so that, if the two mechanisms were of equal importance in the eyes of firms, we would identify a null effect on the experimental coefficient identifying the differences between these two preferences. Third, we randomly assigned respondents to learning that changes were the result of comments by either fellow firms or chemical safety experts. This endorsement

experiment provided a test of the participation spillover mechanism.

Our results provide mixed support for greater quality and efficiency in the regulatory design process through the better law mechanism and for potential cost savings through indirect participation. First, firms were 21% more likely to hold a positive view of a regulatory change if it resulted from feedback by other similar firms even when not told of the participation of their peers. The counterfactual comparison in this analysis was exposure to clauses that were revised through Vietnam's traditional regulatory design process, which does not provide formal access to ordinary small and medium-sized enterprises (SMEs), but is widely understood to allow informal influence by elite, connected firms. Thus, even without explicit knowledge of the source of the change, firms gravitated toward the ideas and linguistic suggestions of their peer firms. This underlines how difficult it is for government to govern firms without their participation. Importantly, greater acceptance of regulatory changes occurred primarily in cases in which firms were prompted to think about the interests of workers instead of their own business motivations. Second, revealing the role of business participation behind these changes led managers to view state regulation, in general, as less corrupt. Learning of the participation by fellow firms did not, however, further improve views of specific regulatory changes.

2. Hypotheses Development

Three main theoretical mechanisms link business participation in the regulatory design process to greater compliance and social protection. First, most broadly referenced and empirically best supported is the *legitimacy mechanism,* which holds that participation improves regulatory compliance by increasing the legitimacy of regulations in the eyes of firm owners and managers (Malesky and Taussig 2017, 2018). This view builds on arguments regarding why individuals obey laws and other rules in the literatures on procedural justice (Makkai and Braithwaite 1996, Kim and Mauborgne 1998, Tyler 2006), deliberative democracy (Fung and Wright 2001, Fishkin et al. 2010), and environmental regulation (Hatcher et al. 2000, Winter and May 2001, Stern 2008). The second, which we label the *information mechanism*, proposes that the act of participation helps firms to better learn and absorb information about specific regulatory previsions (Fitzpatrick and Sinclair 2003). This increased understanding of the substance and logic underlying regulations, in turn, is expected to increase participants' probability of compliance (Blackburn and Hart 2002, Keen et al. 2005).

These first two mechanisms relate principally to participation's effect on participating firms themselves. In this paper, however, our primary mechanism of focus, the substantive change mechanism, involves focusing on alterations to particular regulations and the implications of those changes for business acceptance and compliance. To a meaningful degree, this mechanism is the inverse of the information mechanism with increased learning and absorption by government rather than firms.

A key underlying assumption of the substantive change mechanism is that firms know more about the details of their own relevant operations than do the officials authoring regulations. Owners and managers are better positioned to envision how policy implementation is likely to play out as well as whether cheaper, more effective technological means exist for reaching the intended social protection ends (Stigler 1971, Posner 1974). Officials' limited knowledge about business operations means the state can only imperfectly promote the public interest through enforcement (Spiller 1990, Glaeser et al. 2001, Glaeser and Shleifer 2003). These challenges are particularly pronounced in emerging markets, in which regulatory offices are under-staffed and lack the in-house training and experience necessary to design the vast and varied array of needed new regulations (Kroszner 1998). Determining whether this imbalance in knowledge between firms and bureaucrats is most likely to lead to weaker or better law is critical for clarifying and for testing the substantive change mechanism.

2.1. The Weaker Law Mechanism and Firms' Views on Whether Regulatory Change Benefits Them

The most prominent scholarly work on the political activities of firms supports the weaker law mechanism's darker view of the asymmetric information advantage that firms hold over government. This research focuses on a narrow set of elite firms that strategically use their central roles in the economy and connections to key political decision makers to gain further competitive advantage. Although this can involve pushing for increased state protections that reduce competition (Kreuger 1974, Posner 1974, Buchanan and Tullock 1975), it also frequently entails efforts to remove state requirements that limit commercial opportunities or introduce extra costs for the sake of the public interest. Relevant evidence has accumulated in both the corporate political activity (Hillman and Hitt 1999, Hillman et al. 2004) and regulatory capture (Posner 1974, Laffont and Tirole 1991, Dal Bó 2006) literatures. In each of these academic discussions, the expectation is that government is relatively uncritical in its acceptance of firms' self-interested input. This type of political influence by leading firms is particularly substantial in emerging markets (Hellman et al. 2003, Marquis and Raynard 2015).

The political activities of SMEs, in contrast, have received little scholarly attention. This is despite the fact that such nonelite firms, typically defined as having at most 200 employees, make up more than 95% of firms in the United States and Europe (Organization for Economic Co-operation and Development 2000) and much more in emerging markets (International Labor Organization 2015). This lack of attention to SMEs reflects the reality that business participation in the policy process is indeed generally dominated by a limited set of leading firms.

Nevertheless, it is not difficult to envision that SMEs may be just as likely as elite firms to use any new participation opportunities to undermine state efforts to protect the public. In fact, it is likely that a large share of regulatory input by any type of firm is aimed at reducing the constraints on their business operations. Although it is the job of regulators to carefully balance the benefits of suggested changes against the risks posed to public safety, it is reasonable to be concerned that poorly informed, overworked, and capacity-constrained regulators may be prone to accepting these suggestions without sufficient appreciation of their negative implications for public safety.

We found qualitative evidence for this pattern in the course of looking through the actual comments that firms in Vietnam submitted to the government in response to the specific labor regulation we focus on in this paper. Our particular regulation related to labor safety within firms using hazardous chemicals, a context wherein storing reactive chemicals together can threaten an entire city block or more, whether it occurs in a mom-and-pop shop or a multinational corporation. We hired experienced Vietnamese chemical safety experts to review the choices that regulators made on which suggested changes to this one particular labor regulation to accept and which to reject. We found that it was common for SMEs to ask for exemptions, including submitting that certain clauses were too expensive for firms of their modest size. Even assuming the best of intentions, there was clearly a danger that regulators' lack of technical expertise and insufficient incentives to do the hard work required to overcome their own limitations would lead to inadvisable concessions that higher quality regulators would avoid.

Seen from a purely self-interested firm perspective, if the net effect of business participation is a tangible reduction of transaction costs, such revisions should be viewed favorably by nearly all affected firms. It should not matter whether firms participated themselves in the process. In fact, it should not matter whether they even know about any such participation by firms. This leads to our first hypothesis, which examines the viability of the weaker law mechanism:

Hypothesis 1a. *Even when it is unaware that business input was behind the change, a firm is more likely to believe that a regulatory change represents a positive change for business performance if the change is, in fact, a result of input from other businesses.*

2.2. The Better Law Mechanism and Firms' Views on Whether Regulatory Change Benefits Workers

The better law mechanism holds that business participation in the regulatory design process can facilitate transfer of the superior expertise and experiences of firm owners and managers to regulatory officials. This learning should enable officials to better tailor specific regulatory clauses to withstand the full spectrum of real-world, factory-floor challenges (Stigler 1971, Viscusi and Zeckhauser 1979, Sappington and Stiglitz 1987, Ayres and Braithwaite 1994, Coen 2005, Baldwin and Black 2008). As a result, by contributing to the formation of more relevant and practical government policy, business participation could simultaneously lead to greater regulatory compliance and social protection (Posner 1974).

Critical to arguing for the better law mechanism over the competing weaker law mechanism is the particular form of participation we focus on in this paper and how it differs from the status quo. The weaker law mechanism argument is fundamentally based on a static view of the elite-dominated regulatory capture version of business–government relations that pervades in emerging markets. However, our focus in this paper is the notice and comment form of public consultation (Balla 1998) promoted by the World Bank (2017) and other leading international organizations (Krisch and Kingsbury 2006). This approach aims specifically to counteract the status quo of elite regulatory capture by publicly and transparently posting draft regulations for comment, including by previously disenfranchised SMEs.

The better law mechanism is potentially viable even if SMEs are just as purely profit maximizing as the elite firms that are the focus of the capture literature and, subsequently, focus their input only on weakening of social protections. This is because broad-based consultation across a variety of firm types should reveal a diverse array of competing visions of self-interest and, in so doing, greatly increase state officials' understanding of operational realities. As a result, it should not only improve regulators' ability to foresee implementation challenges from multiple angles, but also increase their power to arbitrate between these and other stakeholder interests in ways that ultimately benefit broader society (Peltzman 1976, Laffont and Tirole 1991). In our research setting of Vietnam, in particular, broad consultation of private SMEs was critical to counteracting the protectionist efforts of state-owned enterprises (SOEs) and paving

the way for the country's much-lauded Enterprise Law in 1999 (Mallon and Van Arkadie 2013).

During the period of our study, the Labor Safety Bureau (LSB) of the Ministry of Labor, Invalids, and Social Affairs (MOLISA), the agency with chief responsibility for designing worker safety regulations for this country of 90 million people and a workforce of more than 60 million, had only seven full-time employees and three interns. Moreover, the committee assigned to draft the hazardous chemicals regulation did not include a chemist or even an expert with enough chemical knowledge to provide us with a full listing of the chemicals defined to be hazardous. Our research team, in contrast, did include local chemical safety experts, who found it very challenging to decipher the first version of the regulation produced by the committee. This view was shared by representatives of the International Labor Organization, who were also providing material support to both LSB and MOLISA.

Based on our interviews, the committee's original research in preparation for writing the draft consisted entirely of two main steps. The first was downloading preexisting hazardous chemical regulations from other countries, including nearby Malaysia. Although this practice of transplanting foreign regulations is common in emerging markets, it is also highly problematic because of mismatches with local conditions. Participation by affected local actors has already been suggested as a critical corrective action for Vietnam in particular (Gillespie 2008).

The committee's second step involved visiting a large SOE and a large foreign multinational that used chemicals. Ultimately, the drafting team made most of the SOE's internal safety standards the law of the land in its first draft of the regulation. This, we were told, is common practice in Vietnam and justified by the superior capacity of SOEs as well as the idea that state ownership elevates consideration of the public good. The SOE, however, was not representative of the larger population of firms using chemicals in Vietnam. In particular, it only used class A chemicals, which produce flames that are extinguishable with water. Consequently, the resulting draft regulation called exclusively for water-based fire extinguishers, ignoring the fact that many firms employed class B and C chemicals that start fires resistant to water. Only after multiple firm comments was the error recognized and corrected.

Had the draft regulation passed, firms using class B and C chemicals would have been noncompliant simply because they chose to employ the proper extinguisher to keep their workers safe. In this particular way, bringing the collective expertise of a broader array of firms to bear on the government's design process clearly enhanced the knowledge of the relevant officials, strengthened the quality of the regulation, and ultimately, made compliance more likely. Critically, increased compliance resulted from improvement of the regulation's fit with operational realities, not reduction of social protections. This improved fit should influence the compliance of firms whether they participate or have any knowledge of involvement of other firms in the regulatory design process.

The preceding discussion generates our second hypothesis examining the better law mechanism:

Hypothesis 1b. *Even when it is unaware that business input is behind the change, a firm is more likely to believe that a regulatory change represents a positive change for society if the change is, in fact, a result of input from other businesses.*

2.3. The Participation Spillover Mechanism

Previous research has provided evidence that firms that directly participate in the regulatory design process are more likely to see state regulation as legitimate and also more likely to comply with resulting social protections (Malesky and Taussig 2018). Nevertheless, designing a national-level program that involves direct consultation of all relevant firms for every draft regulation is too costly to be feasible. If resource-strapped governments in emerging markets tried to reach out to all affected firms for each of the dozens of regulations that they produce every year, the costs of the exercise would quickly swamp any benefits. This is why it is important to explore alternative mechanisms that offer the possibility of exploiting the compliance benefits of business participation at a lower cost.

We begin our exploration for alternatives with the well-established finding in organizational theory that firms frequently mimic the behavior of their peers as they believe that doing so grants them greater legitimacy among important stakeholders (Meyer and Rowan 1977, DiMaggio and Powell 1991). This theory further argues that such mimicry is particularly the case in more uncertain environments (DiMaggio and Powell 1983, Goodrick and Salancik 1996). For example, firms are more comfortable engaging in corruption in emerging markets when they observe their competitors engaging in such behavior (Collins et al. 2009, Lawton et al. 2013).

Identifying and spreading awareness of regulatory compliance is a relatively more difficult task for government to accomplish than is popularizing understanding of its own consultation policies. The hope is that learning that similar firms were involved in the regulatory design process will inspire confidence that the resulting changes reflect a greater understanding of relevant operational realities. This thinking builds on work such as Truex (2014), who applied a similar online survey experiment approach in China and

found that learning about the participation of other citizens improved people's perceptions of the state's legitimacy, particularly among less educated and less connected echelons of society. As yet, however, no work has been done on the behavioral response of firms to participation spillover.

This line of thinking fits within a broader political science literature on descriptive representation that argues that citizens believe their political representatives understand their preferences and are responsive to their views in the laws they create because they share similar backgrounds and experiences (Przeworski et al. 1999, Gay 2002, Urbinati and Warren 2008). Furthermore, learning that government has consulted representative firms in its regulatory design process may enhance their likelihood of believing that regulators truly have societal welfare in mind, leading them to see the state's regulatory authority as legitimate (Goodin and Dryzek 2006, Saward 2006). We term this the participation spillover mechanism for increasing compliance and hypothesize the following:

Hypothesis 2a. *When it understands that other businesses have participated in the regulatory design process, a firm is more likely to see a regulatory change as positive for either business performance or for society.*

Hypothesis 2b. *When it understands that other businesses have participated in the regulatory design process, a firm is more likely to see government's regulatory function, broadly defined, as legitimate.*

3. Research Design and Methodology

This section describes the context, design, and sampling strategy for our randomized online survey experiment involving 121 firms in Vietnam's southern economic hub, HCMC. At the end, we also address two empirical threats to our research design and how we accounted for them.

3.1. Background

Vietnam represents a particularly challenging context for business regulation. On the one hand, it is a country characterized by extreme entrepreneurial expansion. This began with the country's transition from central planning in the early 1990s. Change truly accelerated in 2000, when the government drastically reduced tangible and intangible entry barriers for domestic private firms through the Enterprise Law (Mallon and Van Arkadie 2013). On the other hand, Vietnam's emerging market is still governed by a nondemocratic communist regime that pays its officials very little, allows substantial unregulated economic activity (Tenev et al. 2003), and scores at the bottom of international indices of country governance quality (Transparency International 2018, World Economic Forum 2018). Furthermore, the majority of private firms in Vietnam believe that regulation's primary purpose is to generate bribes for officials (Malesky 2017). In sum, market transition involved government removal of legal obstacles to entrepreneurial entry without a corresponding strengthening of the state's capacity to regulate the growing number of firms in operation, which continue to distrust the motives of state regulators.

Our research design builds on an initiative by the Vietnam Chamber of Commerce and Industry (VCCI) to gather feedback from domestic private firms on this first draft of the chemical safety regulation. This initiative was possible because of the country's 2008 Law on the Promulgation of Legal Normative Documents. Much like an earlier initiative in China, this law required state agencies to post all draft regulations publicly before finalization as part of a 60-day period for gathering stakeholder feedback. VCCI, an organization formed during Vietnam's central planning era and still headed by a communist party member, has an official mandate to mobilize business participation during this process. Implementation of notice and comment in Vietnam, however, has been extremely limited to date (Gillespie 2008), and this VCCI initiative was the first of its kind in the country in its breadth and depth.

The VCCI consultation initiative focused on 11 key clauses. These covered the full breadth of the draft regulation, comprising issues related to all chemicals (storage, handling, factory organization, safety equipment, and transportation) and to specific types of chemicals (flammable, corrosive, deadly). VCCI passed firm feedback on the draft to MOLISA's LSB, the agency with chief responsibility for the regulation's design. Of the 11 clauses, eight were ultimately altered as a result of business participation. We refer to these eight clauses that were influenced by firms throughout the rest of the paper as the "VCCI clauses."

The firms consulted by VCCI were all based in the Vietnamese capital, Hanoi, and 10 surrounding provinces. In contrast, our study focuses on domestic private firms in HCMC, Vietnam's commercial capital. Separated by almost 1,500 km, we believe that HCMC firms were blissfully unaware of both the hazardous chemical regulation and the participation of northern Vietnamese firms in its drafting.

3.2. Sampling and Screening

Our primary goal in sampling firms in HCMC was to identify a similar set of firms to those that participated in VCCI's consultation initiative in the North. This was done to ensure that both the sample of participating firms behind the VCCI clauses we use in our experiment (which, as noted, were derived from the VCCI initiative) and the sample of nonparticipating firms in our online experiment can reasonably

be understood as representative cross-sections of firms affected by the hazardous chemical draft regulation. As a result, on average, participants and nonparticipants should have similar views about the law. This carefully designed setup is what allows us to judge whether a regulatory change endorsed by firms of type A (those randomly selected to provide comments within the VCCI initiative) was accepted by other A-type firms in the population that were not consulted (the sample for our experiment). In other words, in the setting of our experiment, participants and nonparticipants consist of parallel arrays of firm types, both representative of the broader population of relevant firms. This allows us to judge how, on average, firms respond to changes and learning about changes made by firms that are—again, on average—very similar to them.

We drew our sample frame from a database created by Vietnam's General Tax Department (GTD). The full database comprised 129,665 domestic private firms in HCMC and included firm name, CEO gender, number of employees, capital size, and four-digit International Standard Industry Code (ISIC). We narrowed down this set of firms in two main steps. First, we removed all firms in industries that did not relate to hazardous chemicals. Online Appendix A lists which ISIC codes our local chemical safety experts determined to be related. Second, we removed all firms for which the database did not provide a phone number. The result was a final sampling frame of 6,763 firms (see Online Appendix B for details).

We randomly selected firms from this sampling frame to enroll into the study using a randomized block design. This involved generating blocks based on labor size, gender of CEOs, and ISIC code. This blocking helped us reflect the diversity of firms that we learned had participated in VCCI's consultation initiative. This was important because our theory relates to how business participation affects the behavior of similar firms. Blocking is also known to improve efficiency by reducing sampling error in small sample designs such as the one we use in this paper. Firms were drawn from each block to generate an initial stratified random sample of 2,529 firms.

Our team made phone calls to each of these firms to verify that they were still operational, identify their major business activities, and ensure we had the best email address to contact their key decision maker. Calls were made during businesses hours (8:30–17:30, Monday–Friday). Of the 2,529 firms, 1,031 had invalid phone numbers (e.g., wrong number, no longer in service), 209 were assumed bankrupt after not answering at least five attempts, 326 answered but refused to provide information, and 47 had recently filed for bankruptcy or registered but not yet started operations. Removing these firms left us with 916 active firms. Another 380 of these firms, however, were no longer operating in industries that dealt with hazardous chemicals. Of the remaining 536 firms, another 48 firms (9%) either did not have email addresses or refused to provide them because of our plan to conduct an online survey experiment and were dropped from the sample. Ultimately, we invited 488 firms to participate in our survey.

3.3. Survey Administration and Final Sample Size

Our online survey was carried out from July to September 2015 and administered using Qualtrics software. Each firm received an email that included an official invitation letter, a summary of 14 key original and revised clauses of the draft regulation, and a link to the online survey. Online Appendix C shows the official VCCI invitation letter in both its original Vietnamese and an English translation. This letter informed sample firms of the draft regulation and its revision, explained its potential impact on their operations, and invited them to click on to the link to participate in the survey.

Table 1 presents the summary of the original and revised versions of the key clauses that were sent to firms. All of the original versions of clauses were drawn from MOLISA's September 12, 2014, draft, which had not yet been circulated for feedback from chemical safety experts, firm owners and managers, and the public. The matching revised versions consisted of these same clauses as they appeared in the May 2015 draft after adaption by MOLISA to address external feedback.

A day after respondents received emails, we began phone calls to confirm receipt and emphasize the online questionnaire. Our team followed up with each firm for an average of three weeks. In the end, we received 121 responses. This 25% response rate is on par with other surveys of business owners and top managers around the world as well as in Vietnam (Malesky 2017).

Because of the large-scale nonresponse in firms from our original sample frame of 6,763 firms, however, Table 2 tests the representativeness of our final sample against the population represented by the original sample frame. We find that there are no statistical differences in age, size, or legal type of firm. In addition, we find similar numbers of firms in most manufacturing and wholesale/retail industries. However, two significant differences between the sample and general population of firms in HCMC are worth noting. First, our screening procedures generated a larger share of firms working directly in the production of chemicals (23.7%–14.7%). This was because several firms that we originally identified as likely to merely employ chemicals, based on their four-digit sector, dropped out when asked directly about chemicals in their production process.

Table 1. All Clauses Used in Blind Test

#	Clause type	Objective assessment	Old version (March 2014) = 0	New version (May 2015) = 1
1	Experiment	Neutral	Cleaning basin must be equipped right outside the areas of hazardous chemicals to make sure that the workers can wash out chemicals that splash in the body or wash their hands and bodies at the end of their shifts.	Cleaning basin or **water tap** must be equipped **inside** or right outside the areas of hazardous chemicals to make sure that the workers can wash out chemicals that splash on the body **in a timely manner** or wash their hands and bodies at the end of their shifts.
2	Experiment	Neutral	Storehouse of hazardous chemicals must be dry, leak-proof, have lightning protection system, and must be inspected according to the existing regulations.	Storehouse of hazardous chemicals must be dry, leak-proof, have lightning protection system **or situated inside the area that is lightning-proof**, and must be inspected according to the existing regulations.
3	Experiment	Less safe	**Hazardous chemicals must be placed inside a storage place.** Hazardous chemicals must be arranged according to their chemical properties. Do not put in the same storehouse chemicals that can form chemical reactions or have different extinguishing methods.	Hazardous chemicals must be arranged according to their chemical properties. Do not put in the same storehouse chemicals that can form chemical reactions or have different extinguishing methods.
4	Experiment	Neutral	For **merchandise** that is packaged in woven plastic bags, such items must be placed on brackets, shelves at least 0.5 meter away from the wall; hydrophobic chemicals must be placed in a platform that is at least 0.3 meter high.	For **chemicals** that are packaged in woven plastic bags, such items must be placed on brackets, shelves at least 0.5 meter away from the wall; **for** hydrophobic chemicals, they must be placed in a platform that is at least 0,3 meter high.
5	Placebo	Less safe	Dangerous chemical can only be loaded on the devices which are approved by authority and remain within its shelf life.	Dangerous chemical can only be loaded on the devices which are approved by authority.
6	Placebo	More safe	The packaging must be sturdy and sealed. When the chemicals are used up, the packaging must be kept separately. When tire packaging is reused, one has to make sure it doesn't contaminate or change the newly loaded chemicals.	The packaging must be sturdy and sealed. When the chemicals are used up, the packaging must be kept separately. When the packaging is reused, one has to make sure it doesn't contaminate or change the newly loaded chemicals and keep a log book to record the usage of the package whose chemicals are used up.
7	Placebo	Neutral	The empty packages that were earlier loaded with hazardous chemicals must be treated as hazardous chemicals as well, unless it is properly cleaned. Only when both the outer and inner of the packages are properly cleaned, it can be treated as the normal merchandise.	The empty packages that were earlier loaded with hazardous chemicals were only considered normal merchandise after they are properly cleaned. If not, they must be treated as hazardous chemicals.
8	Experiment	Less safe	Specialized trucks for transporting flammable liquid chemicals **must have grounding wire and the sign** "**No Fire.**" The trucks must be equipped with suitable fire extinguishing equipment.	Specialized trucks for transporting flammable liquid chemicals must be equipped with suitable fire extinguishing equipment.
9	Experiment	Less safe	Prohibit welding or other activities that can spark fire near the storehouse **within** 20 meters.	Prohibit welding or other activities that can spark fire near the storehouse **less than** 15 meters.
10	Experiment	More safe	The process of mixing solvents into chemicals in an exposed device must be done at least 10 meters away from the areas which produce flames.	The process of mixing solvents into chemicals in an exposed device must be **shielded from the areas that produce flames or** done at least 10 meters away from the areas that produce flames.
11	Experiment	More safe	The path above devices containing corrosive chemicals must have sturdy barriers and handrails. The equipment and storage tanks must be higher than **the operator position** at least **0.9 meters.** It is prohibited to build the platform in any way or to stack anything that reduces the above height.	The path above devices containing corrosive chemicals must have sturdy barriers and handrails. The equipment and storage tanks must be higher than **the position of the platform where the operator stands at least 1 meters**. It is prohibited to build the platform in any way or to stack anything that reduces the above height.

Table 1. (Continued)

#	Clause type	Objective assessment	Old version (March 2014) = 0	New version (May 2015) = 1
12	Placebo	Neutral	At the storage area of corrosive chemicals, there must have first aid cabinet, water nozzle, container of suitable neutralized chemicals (e.g., $NaHCO_3$ of 3% concentration. CH_3COOH of 3% concentration) for timely troubleshooting in case of accidents.	At the storage area of corrosive chemicals, there must have first aid cabinet, water nozzle, container of suitable neutralized chemicals for timely troubleshooting in case of accidents.
13	Placebo	More safe	Fitted protective clothes made of thick cotton fabric with dust masks against smog must be worn when exposed to toxic dust. When exposed to toxic liquid, workers must cover their wrists, legs and chest. When working with dissolved organic solvents, workers must wear waterproof protective clothing and protective mask.	Fitted protective clothes made of thick cotton fabric with dust masks against **toxic** smog must be worn when exposed to toxic dust. When exposed to toxic liquid, workers must cover their wrists, legs and chest. When working with dissolved organic solvents, workers must wear waterproof protective clothing and protective mask.
14	Placebo	Neutral	The empty packages that were earlier loaded with hazardous chemicals must be treated as hazardous chemicals as well, unless it is properly cleaned. Only when both the outer and inner of the packages are properly cleaned, it can be treated as the normal merchandise.	The empty packages that were earlier loaded with hazardous chemicals were only considered normal merchandise after they are properly cleaned. If not, they must be treated as hazardous chemicals.

Notes. The phrases in bold represent textual changes between drafts. In the "Old Version," they indicate words that were cut. In the "New Version," they indicate words that were added.

Second, we under-sampled firms in transportation by six percentage points (3.4%–9.3%), which is slightly outside the 5% margin of error for our sample size. As with the first difference, this occurred during screening when firms in the transportation sector notified our team that they did not, in fact, transport hazardous chemicals.

3.4 Survey Design

Our survey asks respondents to compare the quality of the original and revised versions of the 14 key clauses highlighted in the original mailing to sample firms. This test encompassed the eight VCCI clauses described in the "Background" section and a comparison set of six additional placebo clauses from the same draft regulation. The drafting committee altered these placebo clauses based solely on input from chemical experts at other ministries, scientific institutes, and academic institutions. The placebo clauses mirror the VCCI clauses in the ways they cover the broad sweep of the regulation and represent the same proportion of objective improvements in labor safety.

Changes from the draft to the final version of the placebo clauses represent our counterfactual comparison group. These changes were made without broad-based business participation and, thus, represent the

Table 2. Representativeness of Experimental Sample

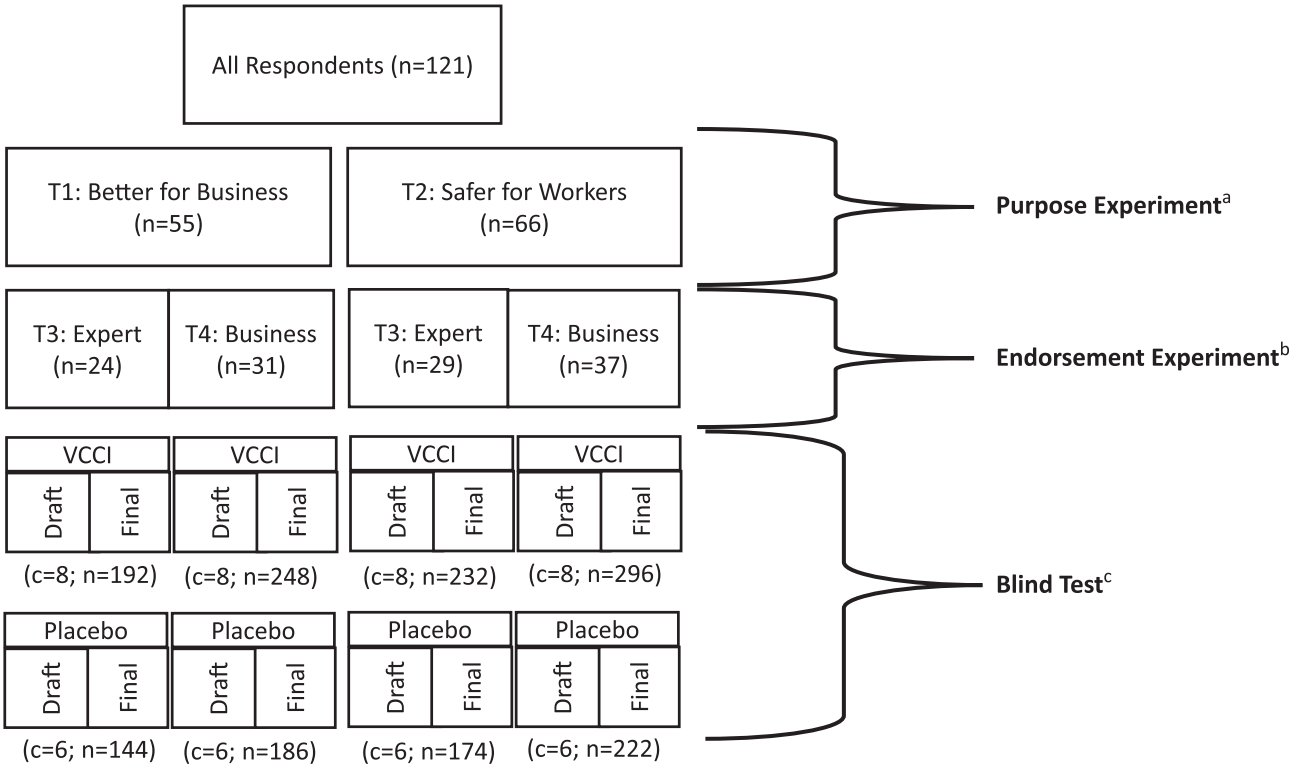
	Sample frame (n = 6,642)		Final sample (n = 121)		Test statistics	
Covariates	Mean	Standard deviation	Mean	Standard deviation	p-value	t-statistic
Year of registration	2008.82	4.40	2008.73	4.59	0.83	0.22
Sole proprietorship, %	6.6	24.8	5.9	23.7	0.78	0.28
Limited liability co., %	77.9	41.5	78.8	41.0	0.81	−0.24
Joint stock company, %	15.5	36.2	15.3	36.1	0.93	0.09
Number of employees, ln	1.53	1.03	1.42	0.87	0.27	1.11
Capital size, ln	21.40	1.79	21.48	1.38	0.62	−0.49
Cl6 (wood products), %	14.8	35.5	12.7	33.5	0.52	0.64
C17 (paper products), %	11.2	31.5	7.6	26.7	0.22	1.22
C20 (chemical products), %	14.7	35.4	23.7	42.7	0.01	−2.74
C25 (fabricated metal), %	13.2	33.9	16.9	37.7	0.24	−1.19
C17 (paper products), %	11.2	31.5	7.6	26.7	0.22	1.22
G (wholesale & retail), %	4.7	21.1	6.8	25.2	0.28	−1.08
H (transportation), %	9.3	29.0	3.4	18.2	0.03	2.19
G (wholesale & retail), %	4.7	21.1	6.8	25.2	0.28	−1.08

status quo for how regulations are normally amended in Vietnam. By comparing views of the VCCI and placebo clauses, we can test whether firms were more likely to select the final versions of clauses that received firm input or those that received the standard expert input. Importantly, our core test did not involve informing firms about the origins of input underlying the changes they were judging. As a result, firms had to base their selection only on their preferences over the text in front of them. In the econometric analysis below, the coefficient on the VCCI clauses represents the difference in the share of firms that preferred clauses with firm input over the counterfactual of input from government experts and the implied informal influence of only elite, connected firms.

In order to test the substantive change and participation spillover hypotheses, we embedded three overlapping research designs into the survey. As Figure 1 illustrates, these three components included (a) blinding respondents to the origins and timing of the regulatory clauses and revisions ("blind test"); (b) a "purpose experiment," alternating whether the respondent was primed to think about business performance (treatment 1 but henceforth T1) or work safety (treatment 2, T2); and (c) an "endorsement experiment" design, alternating whether the respondent was told that the change was the result of input from chemical safety experts (treatment 3, T3) or other fellow SMEs (treatment 4, T4). Figure 1 lays out the relationship between these three components, each of which we now explain in greater detail.

The blind test was a within-firm experiment administered to all respondents, who were "blindfolded" in four important ways. First, they were not told which of the clauses were revised because of firm input (VCCI) or expert input (placebo), allowing us to distinguish business participation from the counterfactual of the local status quo condition of expert input. As noted previously, the status quo is generally associated with crony capitalism, wherein elite, connected firms are still able to exert influence. Second, they were not informed which of the clauses was the original and which was revised. Third, Qualtrics randomly assigned the order in which the two versions of each clause appeared. This meant that original and revised versions of clauses could be found in either of the two

Figure 1. Our Overall Experimental Design



[a]Purpose experiment: To test Hypotheses 1a and 1b, firms were assigned to two different groups and asked to assess which clause in the blind test was either better for business (T1) or safer for workers (T2).

[b]Endorsement experiment: To test Hypotheses 2a and 2b, firms were told that changes in the draft were advised by safety experts (T3) or similar businesses (T4).

[c]Blind test: To generate the dependent variable for the two survey experiments, firms in all four treatment groups were asked to select whether they preferred the draft or final version of 14 clauses from the actual regulation. Eight were clauses advised by firms (VCCI) and six were clauses advised by safety experts (placebo). Firms were not told which was the draft or final or which was VCCI or placebo, and order was randomized.

columns, left or right, of the survey table. Fourth, the numbering of the 14 clauses was randomized to minimize any potential ordering biases.

Unlike the blind test, the two survey experiments were administered in a 2×2 factorial design. This is a technique in which two experimental arms are administered in an overlapping manner that creates four groups. Random assignment to one of these four groups was carried out immediately after respondents clicked "agree" on the online consent form and immediately before they were led to the first page of our survey (see Table 3 for the exact text that respondents encountered on this first page).

For the purpose experiment component, firms were assigned to evaluate changes to clauses based on which version was either "most suitable for successful business operations" (T1) or "provides the greatest protection to your employees" (T2). Our expectation was that T1 firms would view reductions in the stringency and burden of regulations as positive. By contrast, we expected that T2 firms would evaluate the clauses based on which one provided a safer workplace. When we originally designed the study, our assumption was that that changes judged to strengthen workplace protections would also generally be seen as burdensome to businesses. Combining the purpose experiment with the blind test, we use T1 to test the weaker law mechanism (Hypothesis 1a) and T2 to test the better law mechanism (Hypothesis 1b). We further discuss the fit between our hypotheses and test in the "Conclusions" section at the end of the paper.

The third design component is our endorsement experiment. We follow a technique whereby researchers randomize the exposure of firms to different sources of authority in regard to policy (Lupia 1994, Rosenfeld et al. 2016). In our case, we randomly assigned respondents to being told about one of two possible sources of the regulatory changes they were evaluating. One group was assigned to learn that the change was due to "chemical and labor safety experts" (T3), and the other was told that advice came from "over 400 small and medium-sized enterprises" (T4). This manipulation assesses the participation spillover mechanism, whereby learning about participation by similar firms in the drafting process enhances firm perceptions of the legitimacy of the specific regulation (Hypothesis 2a) or government's broader regulatory authority (Hypothesis 2b).

Table 3. Exact Text of Survey Experiment (Two-Dimensional Factorial Design)

Thank you for visiting VCCI business participation portal. Here, you will find the two versions of the draft on the *National Technical Regulation on Occupational Safety in Production, Commerce, Usage, Storage and Transportation of Hazardous Chemicals* that are currently under consideration.

Of the two versions, one was originally proposed by the drafting committee and the other was revised based on suggestions and comments from **chemical and labor safety experts/over 400 small and medium enterprises** [treatment 3/4]. In order to improve the quality of the draft, we ask that you spare some of your valuable time simply select the version that you think is **most suitable for successful business operations/ provides the greatest protection to your employees** [treatment 1/2].

3.5. Item Nonresponse and Balance Across Respondents

We report in Figure 1 that the number of respondents randomly assigned to each treatment condition is not perfectly proportional across the four treatment groups. This is because assignment to treatment condition occurred when the respondent clicked on to the survey rather than during the original sampling process. The computer assigned a random number to each respondent, which was associated with a particular condition at that point. With an extremely large sample, the numbers assigned to each condition would converge; but some disproportionality is possible with a sample of only 121.

More worrisome is the possibility that the assignment conditions themselves caused differential rates of item nonresponse. Different from nonresponse on the overall survey (i.e., not agreeing to click on the survey link at all), this type of bias would show up in the form of skipped questions and higher noncompletion rates. If this were the case, we might observe a form of selection bias that could be associated with the dependent variables in which we are interested, confounding our analysis.

To ensure our results were unbiased, we conducted a balance test of potential confounders in the GTD database in Table 4. Each row in Table 4 represents a regression of a possible confounder (i.e., labor size, firm type, narrow sector, female ownership) on the four treatment conditions. Two findings in the table make us confident that we do not need to worry about differential refusal rates. First, the column labeled "*F*-test" tests the null hypothesis that treatment conditions are significantly different from one another. If any two of the conditions were different, we would reject the null hypothesis. This only occurs in one condition, which is the number of employees. A nonparametric and more precise version of the same test developed by Kruskal–Wallis (Kwallis) finds no significant differences across treatment groups. The bottom line is that assignment appears to have been truly random with no signs of imbalance between groups. Thus, we can still calculate valid average treatment effects for the experiments on the four groups of respondents.

3.6 Perceptions vs. Objective Analysis of Effects of Clauses on Worker Safety

A second empirical threat relates to firms' ability to accurately assess worker safety. A key concern is that

Table 4. Balance in Covariates

	Treatment T1 &T3		Treatment T2 &T3		Treatment T1 &T4		Treatment T2 &T4					
Dependent variable	β	95% CI	β	95% CI	β	95% CI	β	95% CI	*n*	R^2	*F*-test	KWallis
Labor size (four-point scale)	1.522	(1.203–1.840)	1.478	(1.188–1.768)	1.617	(1.333–1.902)	1.514	(1.257–1.770)	118	0.805	0.575	0.80
Number of employees, ln	1.303	(0.939–1.667)	1.347	(1.015–1.678)	1.412	(1.081–1.744)	1.423	(1.122–1.724)	115	0.719	0.050	0.867
Capital size (four-point scale)	3.006	(2.507–3.505)	2.856	(2.402–3.310)	3.171	(2.725–3.618)	3.054	(2.652–3.456)	118	0.867	0.759	0.91
Capital size, ln	1.337	(0.952–1.721)	1.120	(0.770–1.470)	1.387	(1.043–1.730)	1.220	(0.910–529)	118	0.658	0.869	0.84
Located in district 1, %	0.038	(−0.062–0.137)	0.097	(0.006–0.187)	0.163	(0.075–0.252)	0.000	(−0.080–0.080)	118	0.143	3.425	0.70
Sole proprietorship, %	0.239	(0.070–0.408)	0.262	(0.108–0.416)	0.158	(0.007–0.309)	0.216	(0.080–0.352)	118	0.233	0.461	0.89
Limited liability co., %	0.519	(0.308–0.729)	0.549	(0.358–0.741)	0.548	(0.360–0.736)	0.541	(0.371–0.710)	118	0.541	0.787	0.98
Joint stock company, %	0.123	(−0.024–0.269)	0.057	(−0.077–0.190)	0.231	(0.100–0.362)	0.162	(0.044–0.280)	118	0.171	0.859	0.76
Partnership, %	0.081	(−0.016–0.177)	0.065	(−0.022–0.153)	0.031	(−0.055–0.117)	0.081	(0.004–0.158)	118	0.081	2.173	0.98
Owner is female, %	0.202	(0.042–0.361)	0.162	(0.017–0.307)	0.095	(−0.048 - 0.237)	0.243	(0.115–0.372)	118	0.195	0.653	0.78
C16 (wood products), %	0.120	(−0.016–0.256)	0.128	(0.004–0.252)	0.162	(0.041–0.284)	0.108	(−0.001–0.218)	118	0.141	0.726	0.96
C17 (paper products), %	0.246	(0.113–0.379)	0.095	(−0.026–0.216)	−0.003	(−0.122–0.116)	0.162	(0.055–0.269)	118	0.182	2.165	0.42
C20 (chemical products), %	0.242	(0.063–0.421)	0.192	(0.029–0.355)	0.194	(0.033–0.354)	0.324	(0.180–0.469)	118	0.255	0.519	0.82
C25 (fabricated metal), %	0.118	(−0.037–0.273)	0.163	(0.022–0.304)	0.161	(0.023–0.300)	0.189	(0.065–0.314)	118	0.165	0.193	0.98
G (wholesale & retail), %	0.039	(−0.064–0.142)	0.064	(−0.030–0.158)	0.098	(0.005–0.190)	0.054	(−0.029 - 0.137)	118	0.071	0.252	0.98

Notes. Each row represents a regression of the dependent variable on the four treatment conditions. The *F*-test tests the null hypothesis that any one of the treatment conditions is significantly different from the others. The Kruskal–Wallis test provides a nonparametric version of the same difference in means across groups. We report the results of a chi-squared test.

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

the placebo clauses might be of different quality than the VCCI clauses as they were not subject to business input. To minimize such bias, we chose both placebo and VCCI clause revisions that independent chemical safety experts told us represented the full range of possible changes. Table 5 summarizes an objective analysis by these experts as to whether the revisions made workers more or less safe. For instance, by lowering the regulatory distance between chemicals and welding equipment, clause 9 clearly leaves workers in a more precarious situation. By contrast, clause 10, which adds a provision regarding shielding equipment, improves worker safety. Other clauses were revised to enhance clarity but did not recognizably shift underlying worker safety. Their net impact on safety is therefore neutral.

In sum, 37% of VCCI clauses and only 17% of placebo clauses made workers less safe. As a result, if we find statistically significant results in support of our hypotheses, it is despite a bias against a finding that firms would select VCCI clauses as safer than placebo clauses.

4. Results

4.1. Baseline Findings

The *y*-axis in Figure 2 depicts the share of respondents selecting the revised draft over the original. Respondents are subdivided into whether they were exposed to the VCCI or placebo clause at the time of response. This allows determination of whether support for revisions was higher when respondents were exposed to revisions informed by business input even without being told directly that the change came about because of input by other firms. Thus, if a firm selected revised versions for four of eight VCCI clauses, we would observe a score of 50%. The plots reveal a clear difference between the distributions of the VCCI and placebo clauses. On average, about 54% of firms selected the revised VCCI clauses compared with 47% for placebo clauses. In line with the substantive change hypothesis (Hypothesis 1), this indicates that the wording of changes informed by business influence has a greater influence on selection than the wording of changes informed by experts.

However, it is not clear how much can be drawn from this baseline difference, because the graph does not take into account heterogeneity in the types of clauses. Specifically, although respondents are randomly assigned, clauses are not, and every respondent must answer every clause. Therefore, features of the clause may influence selection. As Figure 3 shows, the evaluations of business are also strongly correlated with objective assessment depicted in Table 4.[1] When changes objectively made the law less safe, there is no meaningful difference in selection rates, and firms were generally reluctant to choose the

Table 5. Direction of Revision According to Objective Analysis of Labor Safety (Determined by Chemical Safety Consultants)

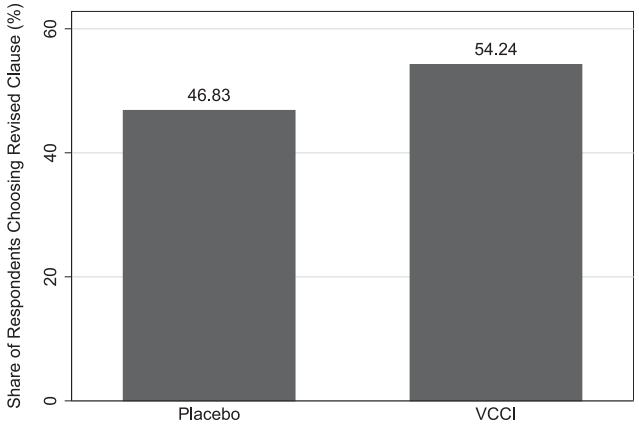
Objective assessment	VCCI	Placebo
More safe	2	2
Neutral	3	3
Less safe	3	1

revised version (29% selected the revised VCCI clause versus 22% for the revised placebo). The opposite is true when the revision objectively made the law safer. In this case, selection of revisions was extremely high for both types of clauses (93% selected VCCI versus 81% select the placebo). Finally, when the experts believed the effects of the revision were neutral, the differences in selection rates are particularly noteworthy (57% selected the revised VCCI clause compared with 32% the placebo clause).

The clear differences in selection by the objective evaluation of the clause has two important implications for our analysis. First, we must directly address the differences in clauses by employing evaluation fixed effects so that our analysis effectively calculates the treatment effects of our experiment within each evaluation type. Second, the statistical significance of our chi-squared tests in Figures 2 and 3 are likely overstated because the respondent-level analysis does not address unobserved heterogeneity at the clause level that can lead to correlation of the error terms among respondents answering particular clauses. To address this issue empirically, we employ mixed-level (or hierarchical modeling) in our regression analysis. This approach takes into account the fact that responses to clause selection are nested within both respondents and clauses.

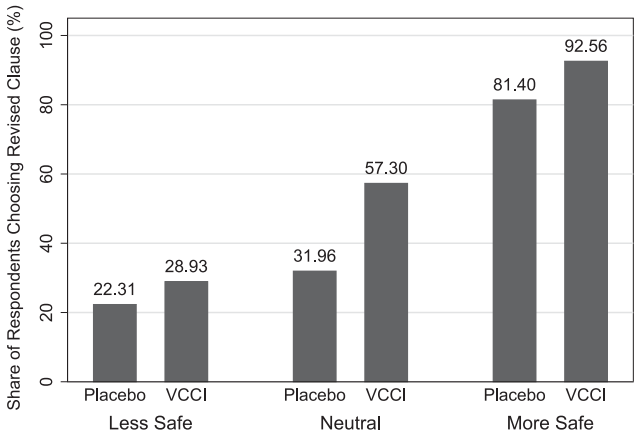
Finally, the bivariate analysis does not address the fact that we asked respondents to assess different

Figure 2. Selection of Revised Clause



Notes. $n = 1{,}556$; 121 respondents, each answering 14 clauses. Difference in selection rates: $\chi^2 = 0.1$, $p = 0.002$.

Figure 3. Selection of Revised Clause Based on Objective Analysis of Effects



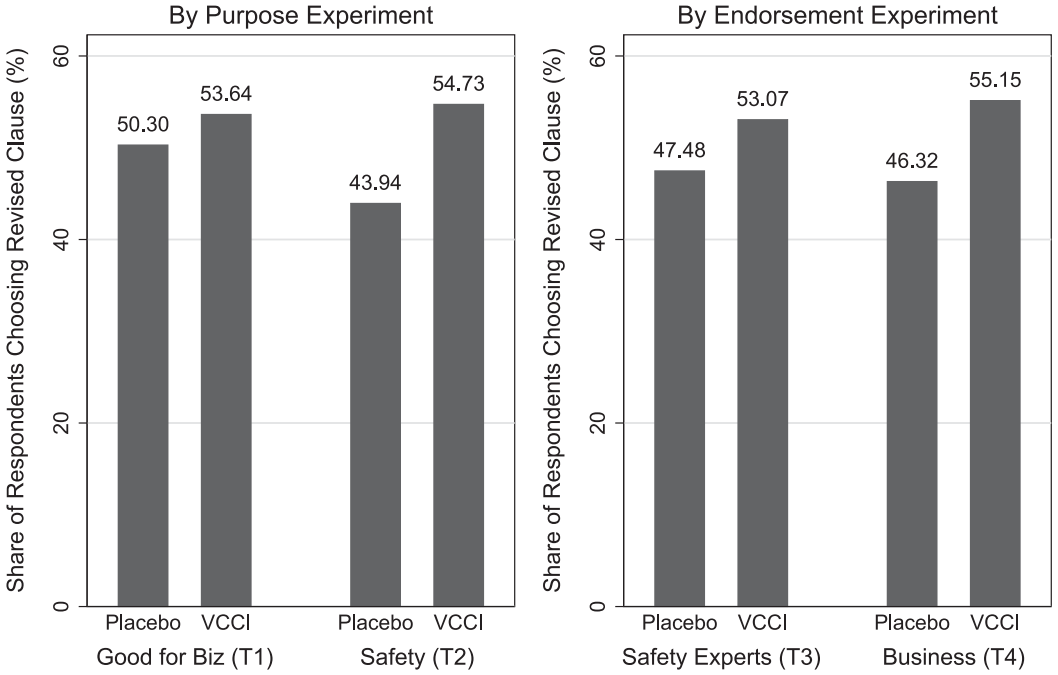
Note. Tests of difference in selection rates: less safe, $n = 484$, $\chi^2 = 2$, $p = 0.157$; neutral, $n = 726$, $\chi^2 = 47.2$, $p = 0.000$; more safe, $n = 363$, $\chi^2 = 7.9$, $p = 0.005$.

types of questions. Some respondents were asked to adjudicate the impact of the revisions on safety, and others were asked to assess the implications for business performance. As Figure 4 shows, these treatment conditions led to substantial differences in the likelihood for a firm to select the revised placebo or the VCCI clause. The statistical significance is likely overstated, as we have not yet dealt with the within-clause clustering, but the substantive difference in gap size is very large. Those told to evaluate whether the revision improves safety (T2) have a selection rate of the VCCI clause that was 11 percentage points higher than the placebo clause. By contrast, those told to evaluate whether the change was good for business (T1) has a selection rate difference that was less than four percentage points. Similarly, the selection gap for those told businesses advised the law (T4) is nine percentage points, marginally higher than the four percentage point difference for those told experts advised the law. In our regression analysis, we explore this issue by interacting the experimental conditions with the VCCI clauses to calculate the conditional effect of the VCCI clause, given different treatment assignments.

4.2 Testing Hypotheses 1a and 1b (1): Regression Analysis

To assess our experimental results more systematically, we restructured our data so that the answer to each question became its own observation. Thus, each variable is indexed at both the clause (q) and individual (i) levels. The dependent variable is coded as one if the respondent chose the revised clause (whether it was assigned to the left or right column). The key independent variable was whether a clause changed as a result of business input ($VCCI_Clause = 1$) or whether it only received chemical safety expert

Figure 4. Selection of Revised Clause Based on Treatment Conditions



Note. Tests of difference in selection rates: T1, $n = 770$, $\chi^2 = 0.84$, $p = 0.359$; T2, $n = 924$, $\chi^2 = 10.5$, $p = 0.001$; T3, $n = 742$, $\chi^2 = 2.3$, $p = 0.132$; T4, $n = 952$, $\chi^2 = 7.3$, $p = 0.007$.

advice (*VCCI_Clause* = 0): δ_i is a matrix of respondent-level control variables that includes the two survey experiments and variables used in the block randomization to increase efficiency (female CEO, labor size, sector), and γ_q introduces fixed effects for whether the question was objectively coded as leading to improved safety, worsened safety, or was neutral.

$$PR(Revised = 1)_{qi} = \beta_0 + \beta_1 VCCI_Clause_q + \delta_i + \gamma_q + u_{qi}. \quad (1)$$

Because of the structure of our data, we apply mixed logit models in Table 6 that cross the respondent and clause levels with one another. This allows us to explore how selection of revisions is nested within both respondents and clauses. Furthermore, all models control for our chemical safety experts' evaluations of whether the change makes the clause objectively less safe, more safe, or neutral. Without this, the effects of business participation would be biased downward.

Model 1 shows the bivariate effect of whether the clause was from a VCCI or a placebo clause. Model 2 adds controls for the endorsement experiment (T4, i.e., input from fellow SMEs = 1; T3, i.e., input from chemical safety experts = 0) and purpose experiment (T2, i.e., safer for workers = 1; T1, i.e., better for business = 0). Model 3 adds blocking control variables and two-digit industry fixed effects. Model 4 controls for whether the change makes the clause objectively less safe, more, safe, or neutral.[2]

The first row of the table displays the odds ratio derived from the logit model for *VCCI_Clause*, indicating that business participation improves acceptability to other businesses even if they are not aware that firms played a role. This estimate is significant at the 0.01 level, substantively large, and consistently estimated in all specifications, including the fully specified Model 3. Converting the odds ratio (2.95) to selection probabilities, respondents had a 59.7% probability of selecting the revised VCCI clause versus a 38.8% probability of selecting the revised placebo clause, a 20.9 percentage point difference in the marginal probability of selection.

This provides strong evidence that participation by firms influences the choice of clauses even when these firms in HCMC had no idea which clauses received business input. In sum, firms were substantially more likely to accept revisions when exposed to wording changes influenced by business than when the wording resulted from the status quo regulatory creation process.

4.3. Testing Hypotheses 1a and 1b (2): The Purpose Experiment

Although the preceding analysis is compelling evidence for the substantive change hypothesis, generally, we still do not know at this stage whether this increase is due to weakening (Hypothesis 1a) or strengthening (Hypothesis 1b) the perceived social protection provided by the regulation. We take this question up in Models 5 and 6 by interacting the VCCI clause variable with T2, the worker safety treatment, instead of T1, the firm performance treatment.

Table 6. Substantive Change: Business Participation's Effect When Participation Is Invisible, When the Goal Is Safety, and When It Is Visible

Dependent variable							
Respondent selected revised draft clause = 1	Substantive change			Weaker vs. better law		Spillover	
	Unadjusted	Experiments	Blocking variables	Safety interaction	Blocking variables	Learning interaction	Blocking variables
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VCCI clause (yes = 1, no = 0)	2.954***	2.954***	2.954***	2.362**	2.364**	2.661**	2.661**
	(1.178)	(1.178)	(1.179)	(0.988)	(0.989)	(1.118)	(1.119)
Purpose experiment (T2 = 1, T1 = 0)		0.887	0.891	0.705*	0.708*	0.887	0.890
		(0.116)	(0.113)	(0.130)	(0.129)	(0.116)	(0.113)
Interaction: purpose experiment × VCCI clause				1.514*	1.512*		
				(0.356)	(0.356)		
Endorsement experiment (T4 = 1, T3 = 0)		1.040	1.055	1.040	1.054	0.938	0.951
		(0.136)	(0.136)	(0.137)	(0.136)	(0.173)	(0.174)
Interaction: endorsement experiment × VCCI clause						1.205	1.206
						(0.284)	(0.284)
Female respondent (yes = 1, no = 0)			0.796		0.796		0.796
			(0.134)		(0.135)		(0.134)
Expert opinion: less safe (yes = 1, no = 0)	0.275***	0.275***	0.275***	0.275***	0.275***	0.275***	0.275***
	(0.122)	(0.122)	(0.122)	(0.122)	(0.122)	(0.122)	(0.122)
Expert opinion: more safe (yes = 1, no = 0)	8.296***	8.296***	8.297***	8.368***	8.367***	8.301***	8.303***
	(4.181)	(4.181)	(4.184)	(4.218)	(4.219)	(4.187)	(4.190)
Labor size fixed effects	No	No	Yes	No	Yes	No	Yes
Two-digit sector fixed effects	No	No	Yes	No	Yes	No	Yes
Constant	0.554*	0.578	0.467	0.653	0.527	0.613	0.495
	(0.193)	(0.210)	(0.228)	(0.241)	(0.261)	(0.227)	(0.245)
Observations	1,694	1,694	1,694	1,694	1,694	1,694	1,694
Number of clauses	14	14	14	14	14	14	14
Number of respondents	121	121	121	121	121	121	121
Log-likelihood	–924.7	–924.2	–918.8	–922.7	–917.2	–923.9	–918.4
Wald chi-squared	36.17	37.01	47.19	39.93	50.02	37.55	47.71

Notes. All columns are mixed methods logit models (using *meqrlogit* command in Stata) with crossed effects at the respondent and clause levels. Coefficients are reported as odds ratios with standard errors reported in parentheses.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

We observe that the odds ratios on the survey experiments are not significantly different from one in Models 1–3, indicating, most importantly, that the purpose experiment has no direct effect on why firms selected particular clauses. However, in this analysis, we want to push beyond the direct effects to learn whether T2 conditions the selection of the revised VCCI clause.

$$PR(Revised = 1)_{qi} = \beta_0 + \beta_1 VCCI_Clause_q + \beta_2 T2_i + \beta_3 VCCI_Clause{*}T2_{iq} + \delta_i + \gamma_q + u_{qi}. \quad (2)$$

In Models 5 and 6, we present the odds ratios from this analysis. The interaction term β_3 provides a key parameter of interest, as it allows us to evaluate the relative salience of the business performance versus worker safety treatments in the receptiveness of firms in HCMC. If β_3 were insignificant, it would indicate that firms do not perceive a difference in the two regulatory goals.

As Model 5 of Table 6 shows, in the fully specified model with evaluation fixed effects, the interaction coefficient is statistically significant at the 0.1 level with an odds ratio of 1.5. This term gives us the ratio of the two odds ratios, implying that the odds ratio of a firm selecting the revised VCCI clause after receiving T2 is 1.5 times greater than the odds ratio of it selecting the revised VCCI clause after receiving T1.

Based on the four component terms of this interaction, and accounting for objective evaluation and blocking variables, we constructed Table 7, a 2 × 2 based on the blind test (VCCI versus placebo clause) and the purpose experiment (business versus safety), to facilitate easier interpretation of these results from the fully specified Model 5 in Table 6. The top left quadrant represents the predicted probability of selecting the revised clause over the original draft clause for firms that received T1, that is, were asked to

Table 7. Predicted Probabilities of Selecting the Revised Clause by Blind Test and Purpose Experiment

Which outcome perspective had the respondent been assigned to consider?	Which type of clause was the respondent considering?	
	Placebo clause	VCCI clause
T1 (firm performance)	42.2%	59.1%
T2 (worker safety)	36.0%	60.2%

think about firm performance and are responding to the placebo clause ($T2 = 0$, $VCCI_Clause = 0$). Based on an odds ratio of 0.527 depicted in the constant and holding all blocking variables and objective valuations at the mean value, these conditions result in a 42% probability of selecting the revised clause. Moving along the top to the right, we next consider firms that also received T1 but are responding to a VCCI clause ($T2 = 0$, $VCCI_Clause = 1$). Their odds ratio of 2.4 implies a 59% predicted probability of selecting the revised clause. Firms that received T2, that is, were asked to think about worker safety and responded to a placebo clause ($T2 = 1$, $VCCI_Clause = 0$) are covered by the bottom left quadrant. For this group, Model 5 indicates a statistically significant odds ratio of 0.7, which, importantly, indicates a slight negative effect. This implies a predicted selection probability of only 36%, the lowest of all four groups. Finally, we have the bottom right quadrant, wherein firms also received T2 but responded to a VCCI clause ($T2 = 1$, $VCCI_Clause = 1$). Based on the 1.5 ratio of odds ratios noted, the selection probability here is 61%.

Taking all four groups together, we can evaluate the marginal probabilities influence of being asked to think about worker safety (T2) on the VCCI clause. When a firm is exposed to T2, the marginal probability of it selecting a revised VCCI clause over a revised placebo clause is 24.2% (60.2%–36.0%). By contrast, when it is exposed to T1, the marginal probability of selecting a VCCI clause over a revised placebo clause is 16.9% (59.1%–42.2%). This 7.3 percentage point difference-in-difference (24.2%–16.9%) in marginal probabilities is statistically significant, substantively meaningful, and supports the better law mechanism (Hypothesis 1b).

Respondents are always more likely to choose the clause influenced by business participation even without any direct knowledge of business influence. However, the positive effect of business participation is particularly substantial when respondents are told that the purpose of the exercise is to decide whether the revisions improved working conditions. In other words, business participation appears to contribute substantively to regulations in ways that improve firm acceptance of worker safety considerations. Indeed, it appears to help firm representatives overcome some resistance to changes meant to help workers.

4.4. Testing Participation Spillover Toward Perceptions of Regulation-Specific Regulatory Quality (Hypothesis 2a)

In the previous analyses, the results hold whether or not firms learned about business participation. In Hypotheses 2a and 2b, however, we suggest that learning about the input of similarly situated business leaders may improve the legitimacy and compliance of the regulation among nonparticipants. Observationally, this increased acceptability should be observable in terms of both the increased legitimacy of the specific regulation that received input and about government regulation, in general. In this section of the paper, we test to see if Hypothesis 2 is upheld in either its specific or general forms.

Models 6 and 7 of Table 6 test for specific effects by taking up the question of whether being informed about business input has any effect on a firm's choice of the revised clause. We already observed in Models 1–3 that there is no independent effect of endorsement. The fully specified Model 7, however, interacts the endorsement experiment ($T4 = 1$ and $T3 = 0$) with $VCCI_Clause$. This interaction term should provide the marginal increase in the probability of selection caused by learning about business participation. In contrast to our expectation in Hypothesis 2a, however, we find no support for participation spillover effects on individual clause selection. The odd ratio is a substantively small (1.52) and not significantly distinguishable from one. Further analysis reveals that this is a true null effect and not the result of an under-powered experiment. Thus, we can conclude that the specific form of participation spillover, described in Hypothesis 2a, is not supported by our test. Business endorsement does not have an independent effect on selection of the revised clause or an indirect effect through acceptance of the clauses that actually received business input.

4.5. Testing Participation Spillover Toward Perceptions of General Government Regulatory Authority (Hypothesis 2b)

To test the outcome of general legitimacy, we employ a dependent variable used in Malesky and Taussig (2017), arguing that an association of regulations with bribe extraction signals doubt that government is primarily concerned with protection of workers, communities, and the environment ($reg_intention$). The dependent variable is based on firm responses to whether they agree with the following statement: "Bureaucrats use compliance with local regulations

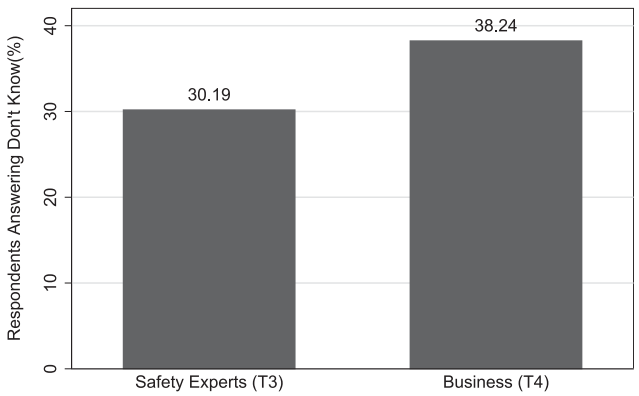
to extract informal fees from business." Firms were asked to answer on a four-point scale ranging from one (strongly disagree) to four (strongly agree), meaning that a higher score implies a more negative evaluation of the intentions behind state regulation. This question is borrowed from an annual firm survey in Vietnam, which has shown that agreement with the question has varied between 40% and 50% over the past 10 years of the survey (Malesky 2017).

One issue with the question is that a large number of firms responded that they "did not know" the answer. Although such an answer may be legitimate, there is also a possibility that this option was selected to avoid answering a sensitive question. Fortunately, as Figure 5 shows, the difference in "don't know" responses is not significantly different across treatments. Nevertheless, the substantive 7.34 percentage point difference is worrisome enough that we also used imputation based on firm size, sector, type, and gender of entrepreneur to predict the missing values (Blackwell et al. 2017). We find that the procedure did not alter the substantive finding, but did increase the number of observations, and thereby the efficiency of the estimates.

Figure 6 presents the direct results of the endorsement experiment, which strongly supports Hypothesis 2b. When firms were informed that businesses advised the decision (T4), they were significantly less likely to believe that regulations were used merely for extracting bribes: 24.5% of T4 firms agreed with the question compared with only 5.9% of T3 firms that received the expert prime. This is an 18.6 percentage point difference.

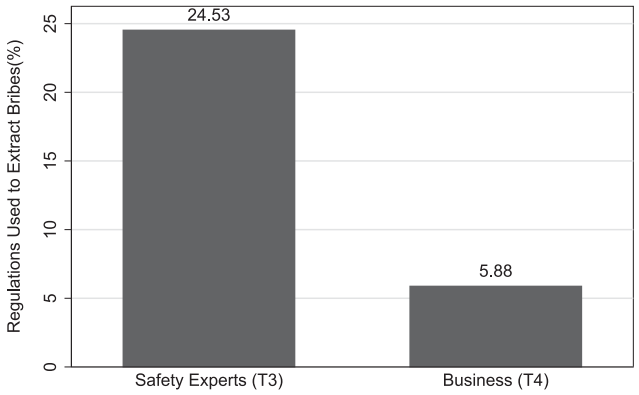
Although these results are persuasive, we also further subject them to multiple regressions to gauge the robustness and precision of the results. This analysis is displayed in Table 8, in which we perform an ordinary least squares (OLS) regression of our *regulation intention* (*reg_intention*) variable on the two experimental treatments and a set of controls (C, all

Figure 5. Respondents Answering "Don't Know" by Endorsement Condition



Note. Tests of difference in selection rates: $n = 121$, $\chi^2 = 0.85$, $p = 0.356$.

Figure 6. Legitimacy: Firms Believing that Regulatory Intent Is to Extract Bribes



Note. Tests of difference in selection rates: $n = 121$, $\chi^2 = 8.6$, $p = 0.003$.

blocking variables used in the original sampling procedure). Respondents are indexed by (i).

$$reg_intention_i = \beta_0 + \beta_1 Endorsement\ Experiment_i + \beta_2 Purpose\ Experiment_i + C_i + u. \quad (3)$$

Model 1 provides the bivariate relationship of the endorsement experiment, controlling for the purpose experiment. As expected, this model recovers the results shown in Figure 6. Model 2 adds a set of controls that were used to create blocks in the sample from the general population to increase efficiency. These include labor size categories and a dummy for female entrepreneur. Model 3 uses two-digit sector fixed effects to make sure that the results are not an artifact of a particular sector's interaction with authorities. These controls increase precision and strengthen the main result. Model 4 tests whether the results are robust to using an ordered probit analysis. Models 5 and 6 replicate the fully specified models but use only the nonimputed data to ensure that imputing data are not biasing estimates.

Models 7–12 repeat the analysis but use a dichotomous measure of the regulatory intent question, which equals one when the respondent agreed (*reg_intention* = 3 or 4) or disagreed (*reg_intention* = 1 or 2) with the statement. Because of the dichotomous dependent variable, we use a probit analysis. Again, the main results are confirmed in all models. In the fully specified Model 10, we find that being told businesses advised the draft (T4) reduced agreement with the statement by 19.3%.

The general form of participation spillover (Hypothesis 2b) is confirmed by this analysis. Learning that other firms helped shape a regulation has a strong impact on improving general legitimacy among nonconsulted firms. They are significantly less likely to believe that regulations are merely the tyranny of red tape, designed to imprison them until they bribe to

Table 8. Legitimacy of Government's Regulatory Authority

	Four-Point Scale					
	Don't know answers imputed				Nonimputed	
	Experiments	Controls	Sector fixed effects	Oprobit	Sector fixed effects	Oprobit
	(1)	(2)	(3)	(4)	(5)	(6)
Endorsement experiment (T4 = 1, T3 = 0)	−0.473***	−0.483***	−0.537***	−1.032***	−0.447**	−0.707**
	(0.121)	(0.124)	(0.118)	(0.224)	(0.193)	(0.278)
Purpose experiment (T2 = 1, T3 = 0)	−0.183	−0.180	−0.192*	−0.380*	−0.153	−0.243
	(0.117)	(0.120)	(0.114)	(0.211)	(0.179)	(0.263)
Female respondent (yes = 1, no = 0)		0.033	0.022	0.038	0.117	0.184
		(0.117)	(0.126)	(0.238)	(0.182)	(0.270)
Labor size fixed effects	No	Yes	Yes	Yes	Yes	Yes
Two-digit sector fixed effects	No	No	Yes	Yes	Yes	Yes
Constant/cut point 1 (for probit models)	2.176***	2.148***	2.360***	−1.575***	2.280***	−1.076**
	(0.113)	(0.140)	(0.191)	(0.373)	(0.294)	(0.440)
Cut point 2				0.221		0.246
				(0.340)		(0.427)
Cut point 3				1.838***		1.744***
				(0.487)		(0.517)
Observations	121	121	121	121	72	72
R^2/pseudo-R^2 (for probit models)	0.135	0.143	0.239	0.133	0.157	0.0748
RMSE / Chi-squared (for probit models)	0.644	0.653	0.629	35.31	0.768	18
Log-likelihood	−117	−116.4	−109.2	−106.3	−76.62	−73.22
	Dichotomous					
	Don't know answers imputed				Nonimputed	
	Experiments	Controls	Sector fixed effects	Dprobit	Sector fixed effects	Dprobit
	(7)	(8)	(9)	(10)	(11)	(12)
Endorsement experiment (T4 = 1, T3 = 0)	−0.187***	−0.199***	−0.222***	−0.193***	−0.224**	−0.256***
	(0.066)	(0.067)	(0.065)	(0.069)	(0.101)	(0.089)
Purpose experiment (T2 = 1, T1 = 0)	−0.043	−0.030	−0.045	−0.036	−0.071	−0.094
	(0.062)	(0.063)	(0.060)	(0.030)	(0.095)	(0.071)
Female respondent (yes = 1, no = 0)		−0.101	−0.123*	−0.066*	−0.122	−0.133**
		(0.062)	(0.066)	(0.034)	(0.106)	(0.053)
Labor size fixed effects	No	Yes	Yes	Yes	Yes	Yes
Two-digit sector fixed effects	No	No	Yes	Yes	Yes	Yes
Constant/PBAR (for probit models)	0.269***	0.260***	0.335***	0.140	0.426**	0.194
	(0.072)	(0.079)	(0.123)		(0.166)	
Observations	121	121	121	121	72	72
R^2/pseudo-R^2 (for probit models)	0.075	0.129	0.243	0.382	0.228	0.306
RMSE/chi-squared (for probit models)	0.338	0.334	0.319	30.59	0.381	18.06
Log-likelihood	−39.1	−35.42	−26.94	−30.35	−26.1	−24.6

Notes. Coefficients with standard errors in parentheses. Models 10 and 12 depict marginal probabilities.
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

be unfettered. Rather, they are significantly more likely to see regulations as a legitimate means to protect workers. Of course, we cannot say at this point whether such legitimacy improves compliance among nonconsulted firms; however, as we noted, there are strong theoretical reasons to believe it might.

5. Conclusions

This study examines two critical questions surrounding calls for greater business participation in the development of regulations in emerging markets and the evidence marshaled in support of these calls. The more controversial of the two is the question of whether a substantive effect of increased business participation on the social protection offered by government is likely to be a net positive or negative. The more practical of the two concerns whether there is potential to leverage the participation of a limited number of firms to affect the behavior of a wider population of similar but nonparticipating firms.

Our results offer encouragement on both accounts. With regard to the first question, we find that firms are more likely to see regulatory changes that came about because of input by other similar firms as improving worker safety even when not told of the other firms' input. This effect strengthens when we zero in on regulatory changes that can objectively be identified as improving worker safety.

As for the second question, learning that business participation was behind a change improved firm perceptions of the legitimacy of state regulatory authority but not of the specific revised regulation. This latter null result was counter to our expectations but may reflect diminishing returns to knowledge of firm input. In particular, the nature of the wording changes to particular clauses may have already clued in respondents to which clauses were altered by other firms. As a result, our definitive revelation of the role of business participation may have been less impactful.

An important strength of the work presented in this paper is that it applies rigorous experimental methods within a particularly challenging empirical context. We reach out to actual owners and managers in authoritarian and nominally communist Vietnam to learn their perspectives on an unfolding set of regulatory changes that promise to have true monetary consequence for their SME operations. Through our careful design, a range of alternative explanations are reasonably controlled for, and there is limited question about the real-world relevance of the study's findings.

Critical to our design was overcoming two major empirical challenges to clearly identify the competing paths by which the weaker and better law mechanisms connect participation to compliance. First, both mechanisms depend on regulators agreeing to alter regulations in line with firms' suggestions. This real-world policy step is usually difficult to observe and essentially always impossible for researchers to control in the field. For example, it is difficult to imagine any government or institutional review board agreeing to random assignment of which suggested changes to accept in a real-world regulatory clause. Second, even if an experimental design were possible, random assignment should result in control and treatment groups being composed of sets of firms with very similar arrays of preferences regarding the regulation. This means that, even if changes engender compliance for certain types of firms, experimenters are still likely to only see null effects as each affected firm type and resulting compliance behavior should exist in relatively equal numbers in both groups. We resolve these problems by using the "blinded" preferences of firms over the regulatory changes in our design, thereby providing a useful measure of prospective compliance.

In working through these obstacles, we believe that this paper represents a significant step forward in showing that management scholars can grapple seriously with even the most empirically challenging questions about business–society interactions. We shed light on the constructive role that business participation can play in government design of regulations that govern their operations. Our empirical setting illustrates that this is the case not only in advanced economies such as the United States with its relatively well-established market and democratic institutions, but also in more challenging emerging markets such as Vietnam with its history of central planning, lack of representative democracy, limited regulatory capacity, and enormous needs for even the most basic forms of social protection.

In fact, business participation in the regulatory design process has a special salience in emerging markets that derives specifically from these countries' relatively less advanced regulatory systems. It is common in emerging markets to rely heavily on existing regulations imported from more advanced economies. However, this practice is also problematic given the inarguable fact that these source regulations are designed to govern businesses operating in very different conditions (Jordana and Levi-Faur 2004, Gillespie 2008, Farrell and Newman 2014). As such, in line with the focus of this special issue, business consultation practices can mediate the implementation problems involved in global regulatory diffusion, tailoring regulations to the local context, which we suggest will ultimately lead to greater compliance by firms and ultimately stronger protection of workers and other citizens.

Naturally, however, the study also has its limitations, and addressing them clearly and honestly helps with both pinpointing the scope conditions of our findings and thinking about paths forward for future research. Chief among the study's limitations is the fact that we rely on the preferences of firm owners and managers as our primary means for judging the effect of business participation on worker safety. That said, interestingly, we do find high correlation between the perceptions of firm representatives and those of experienced local chemical safety experts. Clauses that were judged to be safe for workers by experts were also considered as both safer by business respondents and better for business by matched samples of respondents. Future research on this topic, however, should apply more objective, behavioral measures of the degree to which regulations succeed in their social protection goals.

A second key limitation of our study is the imperfect fit of our broad theoretical framing around the weaker and better law mechanisms with what was empirically testable in our research setting. As we

acknowledge in the introduction, the two versions of the substantive change mechanism introduced in this paper are ideal types that abstract from a reality wherein the two are much more closely linked. As we note in our description of the data, this was reflected in the fact that firm evaluations of whether changes were good for worker safety or for business performance were actually correlated at a level of 0.92. This can be interpreted in at least two ways that are helpful to keep in mind when thinking about firms and regulatory compliance. First, the men and women who lead firms are human beings and thereby are more prone to include social goals in their utility functions than is typically acknowledged by either economics or management research (Bosse and Phillips 2016). Second and relatedly, in reality, what is good for business is not automatically bad for workers and vice versa.

This limitation points to how interesting it would be to explore whether and how the nature of the substantive change mechanism may vary with different firm types. We illustrate just the tip of the iceberg of this complexity when we show that chemical safety experts evaluated some of the changes made by business as having positive effects on society and others as having negative effects. As for firm types, we consider only the very broad categories of the elite, connected firms that influence government informally in the absence of formal government consultation and the ordinary SMEs brought into the process through a broader-based formal notice and comment policy. We do not go deeper into which types of firms preferred safety-enhancing comments and which offered less safe suggestions, but these choices may be related to business size, sector, social connections, and past performance. These differences would be fascinating to explore with a larger, more variegated sample. It would also be very interesting to see work that considers the benefits of participation by nonfirm stakeholders, such as NGOs and research institutes as well as the combined effect of simultaneous participation by other stakeholders and firms.

There is clearly space for significant further unpacking of the impact of business participation on the regulatory design process in future work. The following three related questions are of particular salience to us: (1) What determines *responsiveness*? Specifically, to what input do regulatory drafters pay most attention, and how do they choose which business suggestions to incorporate into their revisions? (2) What determines *effectiveness*? Which types of firms offer comments that are most acceptable to regulators, and what features of the quality and specificity of suggestions make particular comments more likely to influence regulatory design? (3) What determines *behavioral change*? What features of firms make them more amenable to altering their own operations after learning about business consultation in the drafting process?

The third and final limitation we wish to highlight relates to natural questions of the generalizability of our findings, given the specific policy making process in Vietnam. A question along these lines relates to the longevity of the relationships we identify. This originates from our own realization in implementing the project that regulatory drafters actually ignored a great deal of the useful input that they received from all stakeholders, including from firms. This is consistent with evidence in China's more developed consultation practices that lack of regime responsiveness is leading to increased pessimism about the participation process and reducing the quantity and quality of notice and comment participation (Stromseth et al. 2017). As Malesky and Taussig (2017) found in previous work, lack of responsiveness can create a backlash among participants that actually reduces regulatory compliance. Although our findings in this piece are generally optimistic about business participation, it is a single-shot survey and not capable of capturing the dynamic interchange between businesses and regulators over time that may undermine the effectiveness of consultation. Future research should build these dynamic processes into research designs.

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Endnotes

[1] In fact, there is a 0.92 correlation between treatments at the clause level. Online Appendix D presents a scatterplot by share of firms selecting each clause and also shows that firms' safety evaluations corresponded with those of independent chemical safety experts.

[2] Online Appendix E tests the robustness to OLS with respondent-level fixed effects to remove the effects of confounders associated

with the particular personalities or understanding of respondents. As respondents were only assigned to one treatment condition, these are also absorbed by the respondent dummies. Fixed effect estimations employ OLS instead of logit but find substantively similar results.

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